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The Unity of Argument Across Methodological Divides

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Abstract

When one thinks about research as fundamentally argumentative, there are two questions to be asked about any bit of research work: (1) What's the claim? (2) What's the evidence? Different kinds of claims naturally want different sorts of evidence. A belief that one kind of evidence (or method) is intrinsically better than another, independent of the claim at hand, is obviously defective. Thus there is an underlying unity to research methods, in that they are all aimed at offering evidence for claims. Yes, there is methodological diversity in communication--because of the diversity of questions we ask about communication. But it is unhelpful to think of the field as "divided" by method.

The Unity of Argument Across Methodological Divides

My views about methodological divides in the field can perhaps best be summarized this way: I've come to believe that the common methodological "divides" are actually rather shallow--and that emphasizing methodological divisions is at best unhelpful and potentially has undesirable effects.

I should say at the outset that there are lots of ways of distinguishing and identifying methodological variation in communication scholarship--quantitative versus qualitative, social-scientific versus humanistic, and so on--with no great consensus about either the labels or the substantive bases of difference. I'm not especially invested in any particular way of describing these variations, because I think there's an identifiable underlying unity across them all.

That commonality has been made visible to me as a consequence of having come to think about the research enterprise through the framework of argumentation--this, in good measure, encouraged by the work of Sally Jackson (1986, 1989, 1992). When one thinks about research as fundamentally argumentative, there are two questions to be asked about any bit of research work: (1) What's the claim? (2) What's the support (the evidence, the argument, the supporting information and analysis)? And these two questions are broadly relevant, no matter the particular method or topic.

Obviously, different kinds of claims want different sorts of evidence. The evidence that's relevant to a claim such as "Abraham Lincoln plagiarized the Gettysburg Address" is plainly different from the sort of evidence relevant to a claim such as "In the U.S., local television news broadcasts over-represent the proportion of nonwhite criminal offenders." But in each case, the researcher will need to make an argument, make a case for the claim by presenting support.

Approached in this way, research methods can be seen as systematic (methodical) means of generating evidence (evidence for or against claims). But notice that nothing can count as "evidence" except in the context of a claim. No experimental result, no archival document, no ethnographic observation is intrinsically evidence; it has evidentiary value (and status) only in the context of some claim on which it is being brought to bear.

But this in turn implies that the quality of a method cannot be evaluated in the abstract ("are t-tests good methods?" "is ethnography a good method?"). A given method is surely good for some purposes (good for producing evidence relevant to certain types of claims) and bad for other purposes (bad for producing evidence relevant to other sorts of claims). And hence broad evaluative comparisons of methods are simply misplaced. That is, it doesn't make sense to say that (for example) "quantitative methods are better (or worse) than qualitative ones" because the worth of a method cannot appropriately be assessed in the abstract. The worth of a method is given in its utility in generating evidence (for certain kinds of claims).

So: Different kinds of claims want different sorts of evidence. A belief that one kind of evidence is intrinsically better than all others, independent of the claim at hand, is obviously defective. Correspondingly, a belief in the intrinsic superiority of one kind of method (a method for producing a certain kind of evidence) is similarly defective. And all of this can be usefully reinforced, I think, by remembering that methods are, after all, tools--tools for generating evidence. Just as there's nothing intrinsically good or bad about a tool, so there's not anything intrinsically good or bad about a method. Just as one wants to fit the tool to the job, so one wants to fit the method to the claim.

Notice that from this standpoint, there is nothing especially valuable about a research project that uses both (say) quantitative and qualitative methods. I know that sometimes people

seem tempted to suppose that using a diversity of methods somehow makes for an especially good research project--but that's foolish. That's like saying that a construction project that uses both a backhoe and a jackhammer is better than a project that uses only one of these tools. That's nonsense, because one chooses--or should choose--one's tools on the basis of the jobs that need to be done. If you don't need a jackhammer, then it's stupid to change the project so as to have to use one. Having the tools drive the project gets things the wrong way around.

And similarly with research methods: There is nothing intrinsically laudable about a project that uses both quantitative and qualitative methods. Researchers should use methods appropriate for the claims they want to make, but there's nothing intrinsically good about having a set of claims that require diverse methods--nor is there anything defective about having a set of claims that doesn't require diverse methods.

As is probably apparent, from this vantage point, any methodological "divides" look pretty shallow. There is an underlying unity to research methods, in the sense that they are all aimed at offering evidence for claims. Yes, there are differences among methods, but that's entirely appropriate given differences among the argumentative jobs to be done (the different claims to be assessed). (Yes, there are differences among construction tools, but that's appropriate given differences among the jobs to be done.)

So: Why all the hoo-hah about methodological divides? Yes, there's methodological diversity in communication, but so what? (I mean, there's a diversity of tools in building construction, too.) What's the basis for thinking that people are somehow "divided" by methodology?

I think there are several identifiable sources of apparent methodological division. One source is certain unfortunate uses of method as a means of oppositional self-identification, that

is, a process in which one's self is defined in opposition to some Evil Other, where the basis of differentiation is methodological: "I'm a qualitative (or quantitative) researcher--*not* one of *those* researchers."

This practice is unfortunate for at least three reasons. First, once one's identity becomes wrapped up with a particular method, then there's naturally a lot of motivation to elevate that method: "I'm a hammer person, not a pliers person--and hammers are really lots better than pliers, and pliers aren't really any good ..." Such privileging of one method over others can't be justified (and invites unproductive squabbles). Second, such oppositional self-defining can give rise to doubts about the intrinsic merits of the enterprise. If the only way of justifying one's own research is through demonization of the Evil Other Research Method, then one's research enterprise looks weak because there's apparently no intrinsic justification for it. (The only thing that's good about it is that it isn't the sort of thing done by the Other.) Third, this sort of oppositional self-definition encourages a belief in the existence of deep and consequential methodological divides--a belief I obviously think is misplaced.

Another--but more legitimate--reason for supposing that researchers are "divided" by method is the difficulty of genuinely mastering a truly diverse array of methods. This difficulty has two bases. One basis is simply individual abilities and predispositions. If you're not good at math, certain sorts of statistical methods are guaranteed to be difficult for you; if you're not good at managing delicate interactions, certain sorts of open-ended interviews are guaranteed to be difficult for you. The other basis is the time commitment needed for genuine mastery. For mastering a method, there's no substitute for just using it again and again. Mastering ethnographic methods inevitably means doing lots of ethnographic work, for example. I don't mean to say that it's impossible for an individual to master several diverse methods, but it's a

sufficiently difficult and rare accomplishment that we should not think it a realistic goal for most researchers. I also don't mean to say that it's impossible for an individual to have some familiarity with several methods--but familiarity is not mastery. So researchers will inevitably to some degree to be "divided" by methodology, just because people will have varying degrees of expertise in different methods. But that's not a reason to be concerned or upset. (Are we concerned or upset if a jackhammer operator doesn't know how to run a backhoe?)

One final reason for supposing that researchers are "divided" by method is the fact of variation in the substantive research questions of interest. Given the association between methods and claims--that is, given that certain sorts of claims naturally want certain sorts of affiliated methods--one might quite sensibly say that any declared methodological preference actually is a proxy for a preference for certain sorts of substantive questions (viz., those questions best addressed using the preferred methods). So the fact that different researchers use different methods is a natural and proper reflection of the different kinds of research questions pursued.

To be sure, differences in the research questions pursued do divide researchers. Each of us knows that that the most important, consequential, significant, valuable kind of research to do is the sort we do. Other people who are interested in other kinds of questions are obviously wasting their time; it is a mystery why they can't see that what *we* do is plainly more important and interesting than what they do. Ah, well, *de gustibus* and all that.

In sum: Yes, there is methodological diversity in communication--because of the diversity of questions we ask about communication. But it is unhelpful to think of the field as somehow "divided" by method, because in the end it's all a matter of making a case for one's claims.

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